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A COMPLETED CHAPTER IN THE HISTORY OF THE ATOMIC THEORY.*

THE great discovery of the law of gravitation was left reasonably complete by its author. The explanation of this fact is obvi-

* Address by the retiring President of the American Association for the Advancement of Science at the Buffalo Meeting.

ous. No other force of sensible magnitude complicates the action of gravitation; its law appeals to simple geometrical relations; and the facts had been well observed and reduced to order. Accordingly, by a few numerical comparisons of the hypothesis with the facts, Newton established the truth of his conjecture, so that it has been generally accepted as a law of nature. The first suggestion of the theory was quickly followed by its final triumph.

Very different has been the history of the discovery which most chemists regard as next in importance to that of Newton. The discovery that matter consists of an aggregation of infinitesimal units or individuals was made by Dalton; but the first suggestion of this kind had been made at least twenty-two centuries before Dalton. Leucippus and Democritus were the earliest recorded believers in this doctrine; Epicurus adopted it; Lucretius expounded it in strains of noble eloquence. But all the early suggestions were quite barren and unfruitful for the advancement of science, for no one before the present century was in a position to make any verifiable hypothesis; and science grows by means of hypotheses so closely in touch with facts as to be verifiable. In later times, Leibnitz accepted the notion of a certain kind of atomic structure of matter; Newton accepted, and reasoned soundly upon, a view which Dalton recognized as akin to his own. Kant

seems to have adopted the contrary opinion, and to have believed that matter is infinitely divisible. But Bernouilli made the conjecture, which has since been verified, that a given volume of gas consists of a very large number of very small discrete particles, which we now call molecules; and Higgins, an English chemist, a contemporary of Dalton, was the first to apply the notion of atoms to the explanation of chemical phenomena, although he did not think clearly in regard to the weight of atoms, and so formed no useful hypothesis. Accordingly the net result of twenty-two centuries of thought on this subject was to form a conception of a possible structure of matter, without imagining any way of establishing the truth or error of this conception, or even of gaining any evidence whatever in regard to it. But, if any are inclined to visit this failure with reproach, it is interesting to notice that the first man who was aware of the quantitative relations which are adapted to throw light on the matter did not fail to make the most full and complete use of this knowledge.

Dalton, and not the ancients, ought to be regarded as the discoverer of the atomic structure of matter, because he invented a hypothesis, involving such a structure, which was capable of being so compared with facts as to be proved or contradicted; because he actually began such a comparison of the hypothesis with the facts; and because all the evidence from facts, varied as it has since become, supports the hypothesis substantially in the form which he gave it. He who suggests that a certain benefit is desirable, or who conjectures that it is possible, shall not fail of due credit; but he who *confers* the benefit will receive the credit due the benefactor.

Since Dalton's discovery, much has been done to confirm and enlarge our knowledge of the atomic structure of matter. New evidence has been acquired in favor of it,

because the theory has been ready to extend over whole realms of facts of a kind unknown to Dalton, to explain them, to facilitate their study; and also ready to predict facts, unknown till they were sought in consequence of the prediction, but found when they were sought.

The history of the atomic theory for ninety years would fall into several distinct chapters. One of these chapters, not the least interesting of them, would tell of a very large amount of work, some of it of consummate accuracy, of which the object was to attain some knowledge of the nature or construction of atoms. Since the last meeting of our Association in this city, work has been accomplished which, if I rightly judge, has ended this particular chapter. That the chapter may at some future time be resumed is, of course, not absolutely impossible; but for the present it has come to a definite close. My own interest in the matter suggests, and the coincidence in time now mentioned perhaps justifies, my selection of this completed chapter in the history of the atomic theory as the subject of the address which our constitution requires of me this evening.

This chapter naturally concerns more intimately the members of the sections of Physics and Chemistry. To these I can hardly hope to say anything not already well known to them; but members of other sections may, perhaps, not be entirely uninterested in an account of the conclusions reached.

Dalton's theory was founded on three facts. These facts are often called Dalton's laws; one of them, because he discovered it; the others because he first recognized their important relations to chemical theory. One of these is the law of definite proportions: in any chemical compound, the ratio of the components is constant, is invariable, is definite. This truth had been recognized by others; it was finally established as a

result of the discussion between Berthollet and Proust, a discussion well worth recalling for the dignified courtesy and simple love for truth shown by both the disputants. A second of these laws of Dalton is the law of equivalent proportions: if two elements, which combine with each other, combine also with a third, then the ratio in which they combine with each other (or a simple multiple of it) is also the ratio of the quantities of those which combine with the same quantity of the third. That this was true, at least in some cases, was known before Dalton. The third law is the law of multiple proportions: if two bodies combine in more than one ratio, those ratios are simple multiples of each other. This truth was discovered by Dalton.

These three laws are statements of *facts*. Careful and multiplied experiments have convinced us that, if these statements are not rigorously exact, their deviation from accuracy is less than the accidental errors of the best experiments used to test them.

Perhaps it is worth while to delay for a moment, in order to state to what degree of precision such experiments have been brought. The degree of precision with which any supposed law can be verified depends on the skill of the investigator, on the instrumental equipment available, and on the conditions of the problem. Often the conditions of the problem impose very stringent limitations on the precision of our experiments. For instance, the truth known as Ohm's law has been verified, in the case of metallic conductors, to one part in a million millions; but in the case of liquid conductors, the conditions are such that the precision attainable so far has been only a millionth as much. Huyghens' law, relating to double refraction, has been verified to one part in half a million, and there seems to be no possibility of attaining any considerable increase in the precision of the observations. These are examples of the

very highest degree of precision which has been secured in the verification of supposed laws of nature.

The precision which can be attained in chemical analysis, even of the most elaborate kind, is much less than in the cases just mentioned. The determination of atomic weights is the chemical process in which the highest degree of precision is demanded. If we denote the precision of such determination by the words 'good,' 'excellent,' 'admirable,' 'consummate,' then we may fairly say that in a good series of determinations the average difference from the mean of all will be less than one thousandth part of the ratio sought; in an excellent series, less than one three-thousandth part; in an admirable series, less than one ten-thousandth part; and in a consummate series, less than one fifty-thousandth part.

Now the work of Stas was all admirable in precision, and much of it was consummate, and he made experiments expressly intended to verify the law of definite proportions. The average error in this series of experiments was not more than one part in thirty thousand; and his result was, that, if the composition of the compounds examined is not rigorously constant, the variations are too small to be detected. The law of equivalent proportions was verified with the same degree of precision; the accuracy of the law of multiple proportions has been thought to be deducible from the truth of the two other laws.

To some such degree of precision, then, Dalton's laws are the expression of facts. With these facts for a guide, and with no theory founded on the facts and explaining the facts, all chemical computations could be made, and chemical formulæ could be established. And, if a theory should be devised, and accepted, and finally overthrown, these facts would remain, unchanged for our perpetual guidance. Some of Dalton's contemporaries accepted the facts as a suf-

ficient guide, and refused to burden them with the weight of the theory. Some were engrossed, for the time, in following out practical consequences of the facts; some distrusted conclusions supported by but a single line of evidence; some, perhaps, distrusted the capacities of the human mind. But the facts were accepted.

All scientific men, all sensible men, have a great respect for facts. Perhaps one cannot have too great a respect for facts; but his respect may be wrongly directed. Facts are often very interesting in themselves; they often have an important relation to human welfare; their discovery is often a great intellectual triumph; and we may regard them as the miser regards his gold, forgetting that the most precious use of facts is to help us to see beyond them. Facts are evidence; but we seek a verdict. Facts are a telescope; we desire enlargement of vision, further insight into nature. Facts are openings which we laboriously hew in the walls which shut us in; they cost enough to be valuable, but their real value is in that which they promise or disclose. Facts are a foundation for our building; the structure must rigorously respect the lines of the foundation; but it is a pity to believe that the basement walls are the chief beauty desired by the architect or owner. As Tyndall phrased it in a lecture at Manchester, "Out of experience in science, there always grows something finer than mere experience. Experience, in fact, only furnishes the soil for plants of higher growth."

In the present case the soil was fertile, the finer growth has been rapid and vigorous. Dalton inferred that chemical elements consist of very small units or individuals; that all the units or individuals of any given element are equal in weight; and that combination takes place by the grouping together of different units or individuals. This is Dalton's atomic theory.

In Dalton's time there was no fact opposed to this novel conclusion; but there was no second set of facts to support it. The progress of chemistry depended on making due use of Dalton's three laws, and they were quickly and generally accepted; but whether the hypothetical chemical units or individuals actually exist or not, although a most interesting question, did not press for instant decision. Most chemists regarded with favor the idea of the actual existence of the chemical units or individuals. Dalton called them atoms, and perhaps the name brought misfortune; for many thought that the new theory was, that matter is made up of units or individuals which cannot be divided by any possible force. The word 'atom,' the word 'indivisible,' like the word 'individual,' properly mean that which is not divided in the phenomena considered. An absolutely indivisible atom, like an irresistible wave or an immovable rock, can be spoken of to puzzle children, but for adults, as Clifford said, "If there is anything which cannot be divided, we cannot know it, because we know nothing about possibilities or impossibilities; only about what has or has not taken place." I judge that many, probably most chemists and physicists understand the word atom correctly; many others understand it to mean that which cannot be divided by any possible force, and so misunderstand it. For instance, the author of the 'History of the Inductive Sciences' failed to understand the word as chemists and physicists understand it, and so supposed that he rejected the atomic theory. Many chemists would reject the theory that matter consists of very small units which *cannot* be divided. I suppose that very nearly all believe that matter is made up of small units which are not divided in any chemical or physical change yet observed. This is the atomic theory of Dalton.

A few years after Dalton had formed the

atomic theory, and had obtained the first experimental evidence on a matter which had enlisted attention for more than two thousand years, Davy showed, by brilliant experiments, that certain bodies were compounds, although they had resisted all previous attempts to decompose them. Since the first use of electricity had so important results, men were ready to suspect that even supposed elements might ultimately prove to be compounds. It was therefore in a congenial soil that Prout's hypothesis took root. Trusting to experiments of not much accuracy, Prout suggested, in the year 1815, that probably the atomic weights of other elements were divisible, without remainder, by the atomic weights of hydrogen; or, in other words, that they are whole numbers, if the atomic weight of hydrogen be taken as unity.

The new suggestion was most attractive, for two reasons: On the one hand, the truth of the new suggestion would lead to a very great practical advantage. The labor of determining atomic weights would be immensely simplified and lessened if we could know beforehand that the numbers to be found were integers. And, on the other hand, the new suggestion, if approved, would promise a most interesting and valuable hint as to the nature of matter and the structure of atoms. If, for instance, the atoms of carbon and nitrogen and oxygen weigh precisely as much as twelve and fourteen and sixteen atoms of hydrogen, then it is a very plausible hypothesis that each of these atoms is really composed of the material of twelve and fourteen and sixteen atoms of hydrogen, compacted into a new atom. Davy had led many to suspect that perhaps some atoms might be compound, and the new suggestion, looking in the same direction, was received with favor by many, among whom were great discoverers, and great experimenters, and great teachers of chemistry. In England,

where Davy and Prout both lived, Thomson had great influence. It was Thomson who, in the *Journal of Chemistry*, of which he was the editor, first announced Dalton's discovery. Thomson wrote the history of chemistry. Thomson's 'System of Chemistry' was thought worthy of translation into French at a time when French was the mother tongue of chemistry. And Thomson accepted Prout's hypothesis as probably true. But Turner made more accurate and more numerous determinations of atomic weights than any other English chemist; and he rejected Prout's hypothesis. Berzelius, the great Swedish chemist, whose determinations of the atomic weights of all the elements then known were regarded with so much admiration by all chemists, pronounced Prout's hypothesis a pure illusion. But Dumas, than whom none in France stood higher, whose opinion had great weight on account of the excellence of his many determinations of atomic weights, accepted Prout's hypothesis with a slight modification, and believed that his experiments had established its truth. Stas, the distinguished pupil of Dumas, began his work with a bias in favor of the hypothesis; but when his first series of admirable determinations of atomic weights was published, he pronounced the hypothesis a pure illusion, entirely irreconcilable with the numerical results of experiment. But Mallet, who has made several excellent determinations of atomic weights, and Clarke, who has recomputed and reduced to order all the published determinations, declared themselves forced to give Prout's hypothesis a most respectful consideration. It is obvious, then, that ten years ago it was not finally settled whether the hypothesis was or was not true.

The hypothesis, then, has disappointed our hopes of any practical advantage in conducting to a knowledge of the exact value of any atomic weight. But neverthe-

less the hypothesis has not been neglected. As was said, if it is true, we may expect from it new insight into the nature of atoms. Accordingly, an immense amount of labor has been expended in attempting to determine whether the atomic weights of certain elements are or are not divisible without remainder by the atomic weight of hydrogen. Now since our last meeting in this city results have been attained which show that further effort in this direction is not justified by the hope of any theoretic advantage. The chapter has come to an end. Prout's hypothesis cannot be proved by experiment.

When we attempt to decide by experiment whether Prout's hypothesis is true, the nature of the problem, and the limitations of our present knowledge and of our available manipulative skill, impose three conditions to which we must conform.

In the first place, we can more readily test the correctness of Prout's hypothesis by determinations of the smaller atomic weights. The reason is obvious. All analytical work is affected with some accidental error or uncertainty. When Herschel wrote his admirable 'Discourse on the Study of Natural Philosophy' he said that it was doubtful whether we could depend on the result of a chemical analysis as having an uncertainty less than one part in four hundred. Work of much greater accuracy has been done since this statement was made; but, for the moment, let us assume that, even now, the uncertainty of a determination of an atomic weight is a four-hundredth part. This uncertainty affects a large atomic weight much more unfavorably for our purpose than it affects a small atomic weight. For instance, Stas found the atomic weight of lead to be 206.91, if we take the atomic weight of oxygen as 16.00. The assumed uncertainty, one four-hundredth part of this, is 0.53; so that, on our assumption, the true value is some-

where between 206.38 and 207.44. These numbers differ more than a unit; no one has a right, on this showing, to assert that true value is the whole number 207.00, nor that it is not so.

But a small atomic weight may be much less unfavorably affected by the same proportionate uncertainty. For instance, recent determinations show that the atomic weight is 15.88 when the atomic weight of hydrogen is taken as unity. The assumed uncertainty, one four-hundredth part of this, is 0.04; so that, on our assumption, the true value is between the limits 15.84 and 15.92. These numbers differ by only one twelfth of a unit; and both of them differ much from the nearest whole number, 16.00. It is, therefore, by determinations of small atomic weights that we may hope to decide the truth of Prout's hypothesis.

But among the smaller atomic weights, some, in the present state of our knowledge, can be more accurately determined than others. Accordingly a second condition imposed on us by the limitations of our knowledge is that we must determine, with what precision we can, those small atomic weights which admit of the maximum of precision. There are eight atomic weights upon which, with the experimental data now available, the decision of the matter may be fairly made to depend. These elements are lithium, carbon, nitrogen, oxygen, sodium, sulphur, chlorine and potassium; the atomic weights are, in round numbers, 7, 12, 14, 16, 23, 32, 35.50 and 39. If numerous and careful experiments show that these atomic weights are whole numbers Prout's hypothesis has a solid basis in fact; if seven are whole numbers and the other is 35.50, then Dumas's modified statement of the hypothesis has a solid basis in fact, for 35.50 is divisible without a remainder by *half* the atomic weight of hydrogen.

One more condition is imposed on us by the limitations of our knowledge and

manipulative skill. Our experiments determine most atomic weights, not with reference to hydrogen, but with reference to oxygen. Experiment, for instance, does not determine directly that the atomic weight of lithium is seven times that of hydrogen, but that it is seven sixteenths that of oxygen. If the atomic weight of oxygen is uncertain, the atomic weights of the other seven elements, with reference to hydrogen, are all uncertain in the same proportion, although with reference to oxygen they are now determined with very small uncertainty. Accordingly the third condition imposed on us in attempting to learn the truth about Prout's hypothesis is that the atomic weight of oxygen must be well determined.

It may be remarked that it would be a great gain, as all chemists will see, if several other atomic weights could be determined by direct comparison with hydrogen, provided the precision attainable was of the degree which I have called admirable, or even excellent. Now, methods have been devised by which the atomic weights of lithium, sodium and potassium, as well as of several other metals, could be referred directly to hydrogen, by experiments which present no great difficulty and which are capable of the required precision. Further, a method has been devised by which the atomic weight of chlorine can be determined with direct reference to hydrogen, by experiments capable of the required degree of precision, but involving considerable difficulty in manipulation. But, until some such methods shall have been employed by some one, we must be content with the inferences which can be drawn from data of the kind now available, which depend on our knowledge of the atomic weight of oxygen as the corner stone of the system.

Our knowledge of the atomic weight of oxygen ten years ago depended largely on

the experiments of Dumas. His results differed from the whole number 16.00 by one four-hundredth part; he himself judged that the uncertainty remaining might be one two-hundredth part. If we accept this estimate of uncertainty, we may say that he proved that the atomic weight of oxygen is included between the limits 15.88 and 16.04. No one could assert that the true number is, or that it is not, the whole number 16.00. A proportionate uncertainty, therefore, existed in the other seven atomic weights just mentioned. Accordingly, ten years ago we could not well discuss the question whether these atomic weights were divisible, without remainder, by the atomic weight of hydrogen.

The atomic weight of oxygen is, accordingly, doubly important for our purpose. The atomic weight is a small one, well adapted to aid in the solution; and, further, many other atomic weights, also well adapted to aid in the solution, depend on a prior knowledge of this constant. It is for this twofold reason that the work done since our last meeting at Buffalo is important and interesting. The members of this Association have not failed to take upon themselves a fair proportion of the considerable labor involved.

Since that time not less than ten or eleven independent determinations of the atomic weight of oxygen have been successfully concluded.

Cooke and Richards were the first to complete and publish their result; they used a new and ingenious process. Keiser was next; he employed a method for weighing hydrogen which he had independently invented (though it had been previously invented elsewhere) which is the best yet used. In both these series of experiments the hydrogen was combined with oxygen by manipulation something like that of Dumas; but the improvement which permitted the direct weighing of the

hydrogen made the essence of the process novel. Then Noyes devised a new method of weighing hydrogen directly, and a new manipulation for combining it with oxygen, and carried out the process in an apparatus having the advantage of great simplicity. Further, since our last meeting the Smithsonian Institution has published a work containing three series of determinations of the value in question.

In England, Lord Rayleigh used another novel method of combining oxygen and hydrogen, in which he weighed both elements in the form of gas. He also made two series of determinations of the ratio of the densities of the gases. Scott determined the ratio of the volumes of the gases which combine, in several series of experiments of great accuracy. Dittmar and Henderson rendered an important service by repeating, with many modifications, the experiments of Dumas; with the advantage which the later experimenter commonly has over the earlier, they were able to secure a much higher degree of precision and to eliminate the sources of constant error which Dumas detected too late.

In France, Leduc repeated the experiments of Dumas and also determined the ratio of the densities of the two gases.

In Denmark, Thomsen has applied a different process, in which the atomic weight of a given metal is compared with those of oxygen and of hydrogen successively.

We have, then, eleven series of determinations of the atomic weight of oxygen. One of these, for reasons which, so far, are chiefly matter of conjecture, differs much from the mean of all the others. These other ten are concordant; they differ, on the average, only one part in twenty-two hundred from their mean, and the greatest difference from the mean is about one part in a thousand.

Since these experiments have been made by different processes, by different men, un-

der varied conditions, and since the greatest difference from the mean of the whole is only one part in a thousand, it is probable that the mean of all differs from the truth by much less than one part in a thousand. The errors of our experiments are of two kinds—accidental and systematic. If we shoot a hundred times at a mark, about half of our shots fall a little to the right and about half a little to the left. These are accidental errors; accidental errors are lessened as our manipulation improves, and they but slightly affect our final mean. Systematic errors affect all our results in the same direction. Suppose we fire a hundred shots at a target one thousand yards distant, not examining the target, till the shots are all fired. If, now, the sights of our rifle were set for five hundred yards, all our shots would strike too low. This is a systematic error; systematic errors diminish as our knowledge increases.

Accidental errors can be rendered harmless by taking the mean of numerous determinations made by the same method. But systematic errors must be detected and avoided. That they have been detected and avoided in any given case can never be definitely known; it can, at best, be presumed from the fact that experiments by different methods give the same result.

As to the atomic weight of oxygen, accidental errors have now been fairly eliminated, and we can make definite numerical statements on this point. If each of the ten sets of experiments were to be repeated, with the same skill and knowledge, there is not one chance in a thousand that the new mean would differ from the present mean by as much as one part in sixteen thousand. Again, if ten new sets of experiments were to be made by new methods and new experimenters, there is not one chance in a thousand that the new mean would differ from the present mean by as much as one part in twenty-five hundred.

As to possible systematic errors, modesty in statement is incumbent upon all scientific men. But we have now ten independent results in which the difference from the mean is at most only one part in one thousand. We may then fairly assume that the systematic error of the mean is less than one part in one thousand. Again, we have lately been able to take one step in advance, which throws needed light on precisely this point. It has been found possible to weigh some hydrogen, to weigh the requisite oxygen, and to weigh the water which they produce. If, now, there were some undetected systematic error in weighing either one of these three substances, occasioned, for instance, by some impurity remaining undetected in one of them, the sum of the weights of the hydrogen and oxygen would differ from the weight of the water produced. If a pound of sugar and a pound of water produce only one pound and three quarters of syrup, there was a quarter of a pound of sand in the sugar. Now it has, I think, been proved that, if the sum of the weights of the hydrogen and the oxygen is not precisely equal to the weight of the water produced, the difference is too small to be detected, and cannot be more than one part in twenty-five thousand. If there really were a difference of this amount, and, further, if this difference were due to an error at the precise point where it would be the most mischievous, it would render the atomic weight of oxygen uncertain by one part in about twenty-eight hundred.

Taking into account the presumption from the concordance of the results of different experimenters and the presumption from the agreement just mentioned, I think we are justified in assuming that the remaining systematic error is not more than one part in sixteen hundred, and that it probably is not more than one part in three thousand.

If this is a reasonable assumption, the net

results of the experiments made in Denmark, France, Great Britain and the United States is that the atomic weight of oxygen is between 15.87 and 15.89, and that probably it is between 15.875 and 15.885. By no stretch can we imagine that the truth lies in the whole number 16.00, nor in the even fraction 15.50. We cannot sanely believe it to lie in the number 15.75, having modified Prout's hypothesis into the new statement that all atomic weights are divisible, without remainder, by one *quarter* of the atomic weight of hydrogen. It will be obvious that, if we are still resolved to accept some form of the attractive illusion, we must assume that the true divisor is as small as one eighth of the atomic weight of hydrogen, for the value $15\frac{7}{8}$ is included within the limits given.

Then there is one small and well determined atomic weight which utterly refuses to support Prout's hypothesis or any modification yet stated by believers in the hypothesis. Further, now that the atomic weight of oxygen is well established, we can compare, with hydrogen taken as unity, the seven other small and well determined atomic weights which have been mentioned.* We see that every value differs from an integer; for lithium, nitrogen and potassium the difference is about one part in two hundred thirty; for sodium, sulphur and chlorine, about one part in one hundred eighty; for carbon and oxygen, about one part in one hundred thirty. On the average, these values, which are the best determined in chemistry, differ from whole numbers by about one part in one hundred eighty. There is less than one chance in a thousand that these numbers can possibly be so much in error. These are the numbers best fitted to test Prout's hypothesis, and their evidence against it is decisive.

* The values are as follows: Li=6.97, C=11.91, N=13.94, O=15.88, Na=22.87, S=31.83, Cl=35.19, K=38.84.

It ought to be added that the evidence against Prout's hypothesis seemed to many to be decisive, even without the knowledge of the atomic weight of oxygen which has recently been acquired. But the evidence can now be stated in a much more direct and simple manner; and it has gained in force, for to the seven fit instances at hand before there is added an eighth, which happens to be the most weighty of the whole.

In order to present the evidence against Prout's hypothesis when we lack an accurate knowledge of the atomic weight of oxygen, we have first to assume this value. We may, for one trial, assume that this value is the whole number 16.00, which is required by Prout's hypothesis, and see whether, on this assumption, the other seven atomic weights in question are very nearly such as the hypothesis requires.* But the average deviation from the numbers required by the hypothesis is one part in five hundred, and one deviation amounts to more than one part in three hundred. We may make another trial by assuming for oxygen, not the whole number 16.00, but that value which shall make the sum of all the deviations the least possible; and we may also take one quarter of the atomic weight of hydrogen as our divisor.† But the average deviations from the numbers required by the theory is, even in this case, one part in six hundred and the atomic weight of that element for which the determinations of friends of the hypothesis agree with those of its opponents to one part in thirty-five hundred, is supposed, after all, to be in error by one part in five hundred. The atomic weight of oxygen, computed

expressly to give every possible advantage to the hypothesis, differs from the whole number required by the theory by one part in two hundred fifty.

We read in our school books of the bed of Procrustes, to which the tyrant fitted his compulsory lodgers; if they were too short he stretched them on the rack; if they were too long he lopped off the superfluous length. This fable was really a prophetic vision; the bed is Prout's hypothesis; our friends who admire it want to stretch the most unyielding quantities, and to lop off numbers which have been determined with the greatest precision. Either the experiments are in error by an amount which seems incredible, or the hypothesis is an illusion. If the supporters of the hypothesis would avoid the conclusion they must supply better determinations, or they must detect real and tangible sources of error in those already made.

The hypothesis was most interesting and attractive; it promised, if sustained by experimental evidence, to give the means of such insight into the nature of the matter and into the intimate structure of the atoms that it was well worth all the attention which has been given to it. That it should fail of support, that its promises could not be kept, is a matter of regret; but it is time to recognize that our hopes are quite cut off. That other elements are composed of the same substance as hydrogen may or may not be true, but we have now no hope of proving it by determinations of atomic weight. It would not be difficult, perhaps, to modify Prout's hypothesis again and again, so as to bring it into some accord with the facts. We may imagine, if we will, that the observed numbers, if determined without error, would all be divisible by the eighth part of the atomic weight of hydrogen, or the ninth, or the tenth, or by some smaller fraction. But such a hypothesis is of no interest and of no utility, be-

* The values on this assumption are as follows: Li=7.02, C=12.00, N=14.04, O=16.00 (assumed), Na=23.07, S=32.04, Cl=35.46, K=39.14.

† The values are as follows: Li=7.00, C=11.96, N=13.99, O=15.94, Na=22.96, S=31.96, Cl=35.33, K=39.00.

cause it is incapable of proof or disproof by experiment. The reason is obvious. If we suppose that all atomic weights are divisible by one tenth of the atomic weight of hydrogen, then, in case the theory is erroneous, the average deviation of the actual atomic weights from those required by the theory is only the fortieth of the unit. The man who supports a theory which has no physical basis would assert that all such ascertained deviations were due to errors of experiment. Others would reply that you cannot prove that a man is a good marksman by crowding the targets so near each other that not even his random shots can miss them all. But his backers might make so uncritical a claim.

No, Prout's hypothesis, if subdivided far enough, may be true for all which can be proved with the balance; but in such new form it is of no use and of no interest, for it cannot be proved so as to become a safe basis for further inference. In its present form there is no root of truth in it.

So far I have argued that Prout's hypothesis is not true as heretofore enunciated, and that, if some further modification of it is true, we cannot know it. This conclusion has been sustained by the evidence of the chemist's balance. A conclusion supported by a single kind of evidence may command the confidence of one who has been long familiar with the evidence and who has become capable of weighing it. But for others the concurrence of evidence of different kinds rightly adds greatly to its cogency. In this case there is such concurrent evidence. There is other proof that the atoms of some well studied elements are not *additive* structures. Let me briefly describe the nature of this evidence.

When certain elements are volatilized in a colorless gas flame, or in the electric arc, their molecules are made to vibrate, so as to produce light. By the study of this light we can in time learn much of the nature of

the vibrating system. The observed facts are gradually reducing to order, and one result is very striking. In the case of three closely similar elements before mentioned, lithium, sodium and potassium, the complexity of vibration is precisely similar in all, and the numerical relations among the component vibrations are precisely similar in all. Therefore we are compelled to assume that the complexity of structure is the same in all, and that the relations of the component parts, and of the forces acting between them, are the same in all. To illustrate the nature of the argument: the complexity of vibration and the numerical relations among the component vibrations in the case of a large church bell are precisely similar to those in the case of a bell only one third as large. Then, even without the direct evidence of other senses, we must presume that the two bells are similar structures, having similar parts, similarly related. We cannot believe that the larger bell is made of a small bell loaded with weights, nor of three small bells bound closely together. The larger and the smaller are of the same order. The larger is not made of more *parts* than the smaller; it is made of more *metal*. So with the atoms of these three elements; the larger are not made up by the addition of parts which preserve their identity and remain undivided. But all we know of chemical combination relates to structures which are made by the addition of parts which preserve their identity and remain undivided. Then Prout's hypothesis assumes an analogy which does not exist; and deductions from an imaginary analogy will themselves differ from the truth, much as fairy tales differ from history.

There are still other sources of evidence drawn from the specific heats of the elements; the evidence is of the same kind and leads to the same conclusion, but I simply allude to it.

It seems to me, then, that the exact quantitative similarity of the spectra of these elements shows that they are not compounds one of another, subject to the great chemical law of the addition of undivided parts; and that also the magnitudes of the small and well determined atomic weights differ from the values hitherto suggested by applying the law of the addition of undivided parts, and differ by five, ten and fifteen times the greatest experimental error we can reasonably assume.

So the citadel which defends the secret of the atom cannot be taken by way of Prout's hypothesis. We have carried on the assault for eighty years, and we are now satisfied that the way is blocked; we tried to breach, not a wall, but the solid mountain itself. We shall doubtless learn the structure of the atom, but we cannot learn it in the way we hoped. This chapter in our study of the nature of atoms has been fully ended.

If Prout's hypothesis cannot serve us you will doubtless ask what other ways are open by which we may learn something of the structure of atoms. To answer is difficult; to answer adequately is impossible. Perhaps I may mention four lines in which it has been hoped by some that the desired advance could be made, and may indicate what it is reasonable to expect of each.

One of these indications of a possible source of knowledge as to the structure of atoms was suggested by certain chemical observations on some of the rare earths. My brief explanation will not do justice to the conception of the eminent chemist who investigated the phenomena. As I have said, the atom is something which, as a matter of fact, remains undivided in all chemical changes. Most atoms seem to resist every force which we can apply. But it is possible that the amount of resistance which they can offer may vary greatly; it may be that in the case of some elements

the resistance is such that in some reactions the atoms remain undivided, and not in others. From the study of such cases, if there are such, we might expect much help. Now, in the case of the common and well studied elements, the occurrence of such cases has not been suspected; but some of the rarer elements, examined by a process which is frightfully laborious, have exhibited phenomena which suggest, as a hypothesis to be further studied, such a subdivision of atoms. But it is probable that we have mixtures of distinct elements which we do not yet know how to separate from each other by simple analytical processes. This chapter, we may fairly presume, will be valuable; but not because it will tell us anything new about the structure of atoms.

Certain spectroscopic phenomena have suggested that some elements may be decomposed by the action of a high temperature. For instance, it has been thought not impossible that, at the temperature of the electric arc, potassium compounds quite free from sodium should begin to show the spectrum of sodium, because at this temperature potassium is decomposed so as to produce sodium. This hypothesis has been carefully investigated; in part, by the accomplished physicist who is its author; in part, at his suggestion and invitation. It is found that, if years are given to the preparation of potassium compounds free from every trace of sodium, then it is impossible to obtain from them any phenomena suggesting a decomposition into sodium. Here, again, the new chapter, as far as it relates to the structure of the atom, is likely to be but short.

A third suggestion did not rest upon any observed chemical phenomena, but was a purely intellectual creation. This is the hypothesis that atoms are vortex rings in a frictionless fluid. It belongs to the mathematical physicist, rather than to the chemist, to discuss this interesting sugges-

tion. It may be said that it has seemed not impossible that the chemist should find a vortex ring capable of exerting certain chemical forces. But the fate of the hypothesis rested, not with the chemist, but with the mathematical physicist; and it has been found that the theory demands that the weight of a body composed of vortex atoms should increase with rise of temperature. It is scarcely possible that this can be the fact; if, then, the mathematical and physical reasoning involved is sound, it is scarcely possible that atoms consist of vortex rings. The probability is, therefore, but small that we are to learn of the nature of atoms by means of this hypothesis.

Some spectroscopic and other optical phenomena seem to promise more light as to the structure of molecules and atoms, though the dawn is not yet. Thanks to the concave grating, we can determine the frequency of vibration of the light from any source with great accuracy. When the light is complex we can determine, with great accuracy, the relative frequency of the component vibrations. In the cases which have been best studied, the observed frequencies have been reduced to rather simple numerical relations. From the study of these relations we may expect, in time, to determine the structure of the vibrating systems. But the way is long and difficult. Let us illustrate the nature of the method by means of a familiar example, namely, by the study of the structure of a sonorous vibrating system by means of the study of the sonorous vibrations produced by it.

Let us suppose a person deprived of the sense of hearing, but master of the whole mathematical theory of sound. Suppose, further, that he has an instrument which will do for sound what the spectroscope will do for light. With this instrument, let him observe the frequency and the rela-

tive intensity of the vibrations produced by certain musical instruments which we cause to vibrate for him, but withhold from his inspection. Let us, first, sound for him a single note on a piano. The vibrations produced are, as you know, somewhat complicated. Our imagined experimenter, with his instrument, observes vibrations whose frequencies are 100, 200, 300, 400, 500 and 600 in one second; and he also observes that the vibrations of 100 and 500 are of nearly equal intensity, that the vibrations 200, 300 and 400 have more than twice as great an intensity, and that vibration 700 is very feeble. From these facts, if his attainments are sufficient and his imagination sufficiently fertile, he can determine what system produced the sound. He imagines every possible vibrating system—drum, cymbals, trumpet, flute, organ-pipe, harmonium-reed, violin-string, piano, harp and more. Next, assuming each imagined system of such size or tune as to produce one hundred vibrations a second for its gravest tone, he computes what other vibrations will also be produced and what the intensity of each. He finds, for instance, that a closed organ-pipe will give only the frequencies 100, 300, 500, but will not produce the other observed frequencies 200, 400, 600. Therefore, he concludes, the sound we produced for his study is not due to a closed organ-pipe. He finds, after many trials, that the observed frequencies and intensities could be produced by striking a stretched cord with a soft hammer, at a definite point near the end of the cord, so quickly that the cord and hammer remain in contact about the six-hundredth part of a second, and that the observed phenomena could not be produced by any other of the imagined vibrating systems. Then he concludes that the observed sound was probably produced by the stretched cord of a piano. He will have detected the true system, by first imagining every possible

system, by computing the frequencies and corresponding intensities due to each hypothetical system, and by then comparing computation and observation.

For a second example, suppose we ring, for our imagined observer, a bell of a certain form, and that he notes the frequencies 200, 475, 845 and 1295 in one second; in which, also, he finds that the vibration 845 so predominates as to give its pitch to the compound tone. Our observer will not be able to refer this sound to any stretched cord, or to any organ-pipe or other wind instrument; for all these are limited to frequencies contained in the series 200, 400, 600, 800. A uniform metallic bar, suspended and struck like the triangle of an orchestra, will give frequencies not contained in this list, but they will be 200, 550, 1080, and 2670, instead of 200, 475, 845 and 1295. But if our observer has adequate powers he will imagine a hemispherical bowl of suitable dimensions, and will, in imagination, add mass and rigidity in suitable places, until, in time, he will have devised a system whose computed vibrations agree in frequency, and in distribution of energy, with those of the invisible sounding body. Then he would conclude that the observed sound was due to a bell of the form assumed in the successful computation.

This illustration sketches, imperfectly, I fear, the laborious method by which we may learn the structure of a vibrating system from a study of the vibrations produced by it. When we attempt to use this method in order to learn something about the structure of molecules and atoms, our powers of imagination and our mathematical skill are none too much. We know but little which can suggest plausible hypotheses. The facts which are to be explained have been but recently reduced to order. Accordingly, little has been actually accomplished. But there are some few examples of the use of this method

of studying the structure of molecules and atoms.

In one such example the structure imagined consisted of a system of concentric spherical shells, each connected with the adjacent shells by springs. This complicated structure admits of relatively simple computation, and was taken because it fairly well represents a rather simple imagined structure, for which, however, computation is difficult. But it was found that the results computed on this hypothesis gave little promise of agreement with facts.

This was a dynamical hypothesis; it suggested, not only vibrations, but the forces which were to produce them. A second example suggests certain possible motions, but not the forces which might produce the hypothetical motions; it is not dynamic, but kinetic.

As we know, many of the lines in the spectra of the elements are double. For instance, when a volatile compound of sodium is brought into a colorless gas flame, this is colored yellow. When we examine this yellow flame with a spectroscope of sufficient power, we see that there are two frequencies, differing from each other by only one part in a thousand. Now it is probable that these two frequencies are due to the vibrations of one and the same body. There are many illustrations of the fact that a given body may perform two different vibrations whose frequencies differ but slightly. For instance, if we suspend a ball by means of a cord and let it oscillate as a pendulum it is well known that a swing of six feet takes a little more time than a swing of three feet. Suppose, then, that we let our ball swing six feet north and south, and also three feet east and west at the same time; the two motions may be combined so that the ball moves in an ellipse—an ellipse whose longer axis is north and south. If the longer and the shorter swing had precisely the same frequency, the axis of the

ellipse would continue in this direction; but since the frequencies differ, the ellipse slowly revolves. Conversely, from the revolution of an ellipse, we should infer a difference of frequency in the two component vibrations. So it is suggested that the two slightly different frequencies in the light sent out by ignited sodium are due to an elliptic motion in the molecule in which the elliptic orb slowly revolves; this suggestion has not yet been carried so far as to specify any hypothetical cause for the revolution of the ellipse.

These two examples, both due to eminent English physicists, may serve to illustrate the method by which, if I am not mistaken, we are not unlikely to learn much as to the structure of molecules and atoms. We must not expect rapid progress. Even comparatively simple hypotheses may require, for their due examination, the invention of new mathematical methods. And useful hypotheses are rare: like the finding of buried treasures, they are not to be counted on. But, since Prout's hypothesis has rendered us its final service, new hypotheses must be devised, competent to guide us further on our way. Let us hope that, before this city again honors our Association with its invitation to meet here, American chemists and physicists may have had some honorable share in such new advance.

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*PAST AND PRESENT TENDENCIES IN ENGINEERING EDUCATION.**

THE present status of engineering education in the United States is the result of a rapid evolution which has occurred in consequence of opinion as to the aims and methods of education in general. These changes of opinion, whether on the part of

the public or on the part of educators, together with the resulting practice, may be called tendencies. All progress that has occurred is due to the pressure of such views or tendencies; hence a brief retrospect of the past and contemplation of the present may be of assistance in helping us to decide upon the most advantageous plans for the future.

Thirty years ago public opinion looked with distrust upon technical education. Its scientific basis and utilitarian aims were regarded as on a far lower plane than the well-tried methods of that venerable classical education whose purpose was to discipline and polish the mind. What wonderful changes of opinion have resulted, how the engineering education has increased and flourished, how it has influenced the old methods, and how it has gained a high place in public estimation are well known to all. The formation of this Society in 1893, its remarkable growth, and the profitable discussions contained in the three volumes of its transactions, show clearly that technical education constitutes one of the important mental and material lines of progress of the nineteenth century.

Engineering courses of study a quarter of a century ago were scientific rather than technical. It was recognized that the principles and facts of science were likely to be useful in the everyday work of life and particularly in the design and construction of machinery and structures. Hence mathematics was taught more thoroughly and with greater regard to practical applications, chemistry and physics were exemplified by laboratory work, drawing was introduced, and surveying was taught by actual field practice. Although engineering practice was rarely discussed in those early schools, and although questions of economic construction were but seldom brought to the attention of students, yet the scientific spirit that prevailed was most

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